LETTERS TO THE EDITOR.

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The Kinetic Theory of Planetary Atmospheres.

In the paper which I communicated to the Royal Society on April 5, I examined the logical conclusions obtained on the hypothesis that the atmosphere of a planet is distributed according to the generalised form of the Boltzmann-Maxwell distribution applicable to a gas in a field of external force, with the further generalisation required to take account of the effects of axial rotation. As regards the effects of the planet's attraction on the distribution of density, the expressions assumed to represent these were of the form now generally accepted by writers on the kinetic theory (*e.g.* Watson and Burbury), and the modifications required in taking account of centrifugal force were investigated by me in 1894, and are in harmony with the conclusions to which Maxwell's investigations tend. In the aforementioned paper I showed how to calculate a superior limit to the rate at which a planet is losing its atmosphere, and obtained the results that helium would be permanently retained at all ordinary temperatures by terrestrial gravitation and vapour of water by the gravitation on Mars; conclusions with which those deduced by Mr. Cook would appear to be identical, so far as I judge from his letter.

The objections which naturally suggest themselves to the mode of treatment in this paper are that the distribution in question is that which would be brought about exclusively as the result of molecular encounters, and of the free paths of the molecules between these encounters; and that it therefore represents the distribution in an atmosphere of uniform temperature. In an actual atmosphere the equilibrium of the lower strata is largely modified by convection currents, so that the adiabatic law, rather than the isothermal law, is applicable. This point I hope to discuss at full length in the second part of the paper; in the meanwhile, it is hardly likely that any one will suggest that helium escapes from our atmosphere because the upper strata are at a *low* temperature, but that it would cease to escape if the upper strata were heated up to the same temperature as the lower ones. The point at issue between Dr. Johnstone Stoney and Mr. Cook and myself appears to be how far the Boltzmann-Maxwell distribution represents what happens in the *upper* strata of the atmosphere. To assert "that in the present state of our knowledge it" (the *a priori* method as Dr. Stoney calls it) "cannot be made to furnish a valid investigation," seems to me tantamount to striking at the very foundations of our kinetic theories of matter. It may be that these theories will not resist such an attack, but the consequences of the onslaught cannot be properly traced, except by making mathematical determinations in the way that I have done. It appears to me to be just in this very problem of planetary atmospheres that the fundamental assumptions of the kinetic theory are least open to objections. Experiments on the relation of diffusion to temperature led Maxwell to abandon the notion that the molecules of a gas behave as elastic spheres and to consider the effects of finite intermolecular forces. So far as I am aware, (1) every attempt at a kinetic explanation of the thermodynamical properties of gases on the latter view involves some assumption which restricts its validity to the limiting case of attentuated gases, where the number of molecules within each other's sphere of influence is a negligible proportion of the whole number, and the duration of an encounter is negligible in comparison with the time of free motion between encounters. On the other hand, (2) it is amply proved by Watson and Burbury that the Boltzmann-Maxwell distribution, if it hold at any instant, will hold at all future instants in the *absence* of molecular encounters. (3) Boltzmann's minimum theorem tells us that if encounters take place at random, the molecules tend towards the distribution in question. (4) We are told on good authority that we must regard the Boltzmann-Maxwell law as a theorem in probability. Now the divergence between actual conditions and the assumptions required under heading (1) gets less and less as we ascend in the atmosphere; (3) gives us reason for believing that the Boltzmann-Maxwell distribution holds at the highest altitudes where encounters not unfrequently take place; (2) shows that the molecules which are projected from these strata and ascend to still greater altitudes without encountering other molecules remain distributed according to the same law; and (4) removes the necessity of taking the size of the element of volume $dxdydz$ into account by telling us that the law represents not merely the number of molecules having given limits of velocity occurring in the element, but also the *probability* of a molecule coming within these limits, and this probability may be as small as we please.

If helium really does escape from our atmosphere, either there must be a fallacy in the assumptions underlying (1), (2), (3), or (4), and this fallacy must affect numerous previous writings on the kinetic theory, or else our preconceived notions as to the relation between temperature and kinetic energy are at fault. With regard to (4), it may be objected that the error-law fails to apply to events of exceptional occurrence, and therefore that we cannot apply it to calculate the probability of a molecule escaping from the atmosphere when the velocity required would represent an abnormal divergence from the mean. This point was carefully considered by me. It appears, however, to be the accepted view that abnormal divergences are excluded because in practice they never occur, not because their occurrence is far more frequent than the error-law would lead us to suppose. If the methods of the kinetic theory should prove to be inapplicable to rarefied gases as well as to dense assemblages of molecules, and they do not altogether agree with experiment for distributions of intermediate density, the position is indeed a serious one. In face of such a possibility, instead of abandoning our mathematical calculations we ought to push them to their ultimate consequences, in order to arrive at a better understanding of the true state of the case. The escape of gases from the atmospheres of planets is a phenomenon probably more directly dependent on the translational kinetic energy of the molecules than any other property of gases. The prevailing doctrine that not only is the mean value of this translational kinetic energy proportional to the absolute temperature, but the conceptions of temperature and kinetic energy are physically identical, has always seemed to me to require closer investigation than it has as yet received, and it may well be that the kinetic theory of planetary atmospheres furnishes one means of putting this doctrine to a test.

Plâs Gwyn, Bangor, May 26. G. H. BRYAN.

The Severn Bore.

No one who suffers from scientific curiosity should miss seeing a tidal bore at least once in his life. The locality and conditions under which the Severn Bore can be seen make it an ideal object for a pleasurable excursion. The time to be selected is about twenty-four hours after new or full moon; the largest spring tides should be chosen, if possible, and an occasion when the light permits both evening and morning bore to be seen. They occur at about 7.30 to 9 o'clock, a.m. and p.m. The visits should therefore be either when the days are long or at full moon. During a recent excursion, I stayed at Newnham-on-Severn, below Gloucester. This is about 3 hours 20 minutes from Paddington station, and it is possible to leave this station at 3.15 p.m. and be in time for the evening bore, see the morning bore next day, and be back at Paddington by 2.20 p.m.

On April 29, twelve hours after full moon, I awaited the bore at the south-east corner of Newnham Churchyard. The position is the summit of a cliff situated on the outer bank, and near the centre of the base of a U-shaped bend of the Severn, the limbs of the U being four miles long, and the width between the limbs two miles. The prospect is one of the most pleasing in the South of England; the broad, winding river, emerald pastures abandoned by the wandering channel, miles of rich champagne country, with apple and plum orchards, and the distant range of the Cotswolds. At 6.45 p.m. the bore was sighted as a line of white foam between Aure and Fretherne, rather more than three miles down the river. For a quarter of an hour I watched its march up stream, first wheeling by the left, then advancing up the straight reach, and finally wheeling by the right round the last bend. The wheeling movement is most fascinating to watch. I now hurried down to the ferry, and shoved off the boat into deep water to meet the bore, which was now roaring like a railway train. The water channel was about 200 yards wide; at high water it is double that width. On the sands of the opposite convex, shallow shore the bore discharged itself obliquely as a curling breaker. Against our rocky shore it was a bursting surge. A rise of level was perceptible about ten yards in front of this. In the deep channel